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ABSTRACT

Ethnographic methods can be a valuable supplement to our understanding of the limits and possibilities of social science research by addressing the question of subjectivity directly. When ethnographers enter a social situation, they explain themselves and participants see them primarily in the role of fieldworker. But field researchers also have latent roles, such as learner (which may be affected by gender, age, or race), or an identity associated with the setting which has led him or her to research. The participant observer role also explicitly involves participation, including forming relationships with other participants. Part of the form of ethnography is that ethnographers should investigate and report the ways in which their visible latent roles and their emergent roles as fieldworkers shaped others' responses to them. (BW)

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The Impact of Ethnographers' Roles on the Research Process

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In the last few years, it has become increasingly clear that traditional survey research can not produce the kinds of data which illumine processes within schools and school systems. Qualitative research, or the ethnographic method, has consequently enjoyed a sudden burst of moral and financial support. A clear need has emerged both in the context of evaluation research (Farrar, Desanctis, and Cohen, 1980) and in that of basic research (e.g. Ogbu, 1973; Rist, 1979; Rutter, et al., 1979; Willis, 1977) for research that can explore subcultural assumptions, unarticulated regularities in interaction, nonverbal communication, and the development of relationships and identities through time (Rist, 1973, 1978). Nonetheless, many educational researchers and practitioners distrust the "subjectivity" necessarily associated with qualitative research.

It will be my argument in this paper that just as ethnographic methods can be a valuable supplement to quantitative methods in their capacity to portray phenomena inaccessible to the most sophisticated survey interviewer, so also they can be a valuable supplement to our understanding of the limits and possibilities of social science research by addressing the question of subjectivity directly. For those who go to the roots of ethnography, or more broadly of qualitative methods

of research, in anthropology and sociology, it is evident that subjectivity is an inherent part of the research process.

In anthropology, especially, it is a common assumption that the researcher is an instrument of the research, in fact the major instrument. As a human being, the instrument is variable and fallible. One corrects for those faults not by trying to cleanse the research of them, but by giving a full and fair accounting of the instrument and its use so that readers may perceive and allow for the various biases which will inevitably creep into both the field work and the later interpretation of it. The researcher him or herself has a duty to engage in considerable self-analysis in order to discover the ways in which his or her cultural and individual biases may shape either the actual behavior observed or his or her account of the behavior and its meanings. Thus anthropological studies not only routinely include a self-conscious narrative of the conduct of the research which tells the reader something of the researcher's personal characteristics, but they also weave through the account of the data the researcher's part in various conversations and incidents relevant to the analysis (e.g. Spindler, 1973; Wax, et al., 1964; Wolcott, 1973). Thus, the reader gains some knowledge of the instrument used for measurement in each study. There are also whole monographs and collections devoted to discussions of issues such as gaining acceptance in the field, building informative relationships with the people one studies, and interpreting the data of interaction faithfully and insightfully after one leaves the field (e.g. Bowen, 1954; Golde, 1970; Habenstein, 1970; Wax, 1971).

Anthropologists generally give fuller accounts of this kind than do sociologists. The philosophical presuppositions of the field are more hospitable to it, but also the isolation of early anthropologists for months at a time among remote peoples forced them to participate fully in the life of the people they studied and thus to acknowledge their own interaction as part of the data they collected. Sociology has a more positivist tradition. In fact, the main stream of sociology is inhospitable to qualitative research, and qualitative researchers have often been ambivalent about the treatment of their subjective persons as research instruments. They sometimes attempt to set them aside and report what they saw, but little of the lense through which they looked.

However, there has grown up within sociology a tradition of reflection upon participant observation parallel to the one in anthropology. Sociologists working in communities have reflected at length upon their experiences in the field both in monographs, (e.g. Whyte, 1955; Gans, 1962) and in collections of essays (e.g. Vidich, Bensman, and Stein, 1964). And sociologists working in a great variety of complex organizations, have reflected on issues general to research in organizations and specific to particular ones. By reading this literature on research in mental hospitals (Schwartz and Schwartz, 1955) in police departments (Van Maanen, 1981), or in large corporations (Goldner, 1967) one can glean a rather full picture of the way sociologist fieldworkers have developed research roles. The task involves a combination of careful planning, trial and error, and response to organizational constraints.

A smaller literature explores the ways in which the field researcher's interpretation and analysis are affected both by experience in the field and by the full array of social roles and moral commitments which the researcher brings with his or her person to the task of understanding the reality in question. There are issues here both of understanding the data (Douglas, 1976; Gans, 1968; Miller, 1952; Seeley, 1964; Stein, 1964) and of reporting it (Becker, 1964; Colvard, 1967; Moore, 1967; Van Maanen, 1981; Warren, 1980).

In this paper my primary intent is to discuss the ways in which a field researcher's work is influenced by the roles that come into the field as part of his or her person, by the role of fieldworker itself, and by the social, intellectual and moral commitments built up in the many roles of the whole person who analyzes the field data and writes the public report of it. It will be my argument that all of these influences can be both helpful and harmful to insightful and accurate "objective" reporting. I will draw upon the literature of field work and upon my own recent experience in a field study of three schools.

I studied three public middle schools which were designated as "magnet" schools. Each had a different educational specialty, and each drew students on a voluntary basis within racial quotas from throughout the large city of Heartland (a pseudonym). My purpose was to understand the organizational functioning of each school in the light of two questions. First, I attempted to understand the character of each school's special educational approach and the changes in traditional school organization which that approach engendered--or the alterations

necessitated in the approach by the intractability of organizational variables. Second, I considered the way all of the schools were affected by the special relations their magnet status created between them and outside groups such as the court, the rest of the school system organization, and parents.

To this end I needed to understand process as well as structure in each school quite thoroughly. I spent roughly a semester in each school, observing classes, talking informally with participants, attending meetings, reading documents, and conducting semi-structured interviews with adults and children. I also needed to know something of formal and informal politics in the district as a whole. I therefore interviewed parents, listened to school board meetings, and watched both the papers and the informal discussions of the schools which came up around the community. In other words, I behaved as a very typical ethnographer, whether sociologist or anthropologist. My sociological training and my interest in organizations as the unit of analysis focused my attention more on the adults and more on the interplay of formal and informal structure and process than would a different intellectual background.

Latent Roles of the Researcher

When ethnographers enter a social situation, they explain themselves in terms of the purposes of the research and the activities in which they wish to engage to collect data. Participants in the situation will thus respond to them primarily in the role of fieldworker, one which will usually have to be interpreted and indeed built up since it is likely to be unfamiliar to the regular participants. But

the field researchers enter the field also as human beings bringing in their very bodies a set of statuses such as those of gender, age, and race which imply expected role behavior as they interact with other people in the situation. We may refer to these roles which are not part of the primary role upon which interaction is focused, but which may have very important consequences for acceptable and unacceptable patterns of interaction, as latent roles.

In many field situations, the most effective generalized role to which researchers may assimilate their field work roles is that of learner. They are after all there to learn, and participants in many circumstances feel most comfortable in explaining the - to them often self-evident - facts of their social life to some one clearly in need of fundamental instruction. Occasionally, however, especially in dealing with persons fairly high up in formal organizations, the field researcher needs to demonstrate high status, competence, and a prior cache of rarely shared knowledge (Daniels, 1967; Goldner, 1967).

Researchers' age and gender will affect the ease with which they fit into these two styles of relationship. Young or female fieldworkers have roles which induce others to explain the world to them, expecting little opinionated comment in response. Older and male workers are more likely to be expected to be knowledgeable and the norms of interaction suggest a more equal exchange of information and opinion. Thus young females can more easily slip into situations with little effect and can elicit information as learners without inconsistency. But they may have more trouble getting full explanations from persons placed well up in a hierarchy who are more fully conscious of the uses of

information as power. Older male researchers should have the opposite set of problems. Of course, the way one presents oneself can either underscore or counteract the presumptions about one's status and personality based on age and sex.

These characteristics are not altogether external ones, either. Most of us learn behavior in accordance with our gender roles. In my earliest research I found that the early training which had enabled me to be an attentive listener to a "date" was equally useful in being an attentive and appreciative listener to an informant. And the expectation that I would listen merely to be informed, and not to respond, which sometimes annoyed me in my male fellow students was a useful one in my relationship with male informants, especially when I found them hard to empathize with.

In that earliest research (Metz, 1978b) undertaken in 1967-1968 when I was in my late twenties, I appeared even younger than I was. I also came into schools with a training in sociology and no prior experience in schools; so that I was able to present myself very genuinely as ignorant about schools and in need of a good deal of basic tutelage. Many of the older teachers informed me as they would a student teacher.

In my more recent research started almost eleven years after the first field work was concluded[1] I had acquired a doctorate, considerable experience in teaching at the college level, a good deal more knowledge about sociology and some more about schools, two children of my own, wrinkles, and a greater measure of confidence and assertiveness. At the first school I entered, the principal's questioning revealed that

I had a doctorate. She introduced me to some teachers as Dr. Metz, and the title stuck with some despite the fact that I consistently introduced myself as "Mary Metz" or occasionally Mrs. Metz. It seemed to me that this title inhibited the staff and cast me in the role of evaluator more than learner. At later schools I therefore told the principals that I preferred to be introduced as Mrs. or Ms. Metz, according to common school use of titles. It was easier to take the learner's role at these two schools. But I never felt that I was as easily assimilated into this role as I had been when I was younger--and when my not always controllable expressions were perhaps more wide-eyed. Subjectively, I was also less patient in the long hours of observation and quicker to draw conclusions, perhaps to leap to them.

It is important in this context, that as magnet schools, the schools I was studying were also probably more politically vulnerable than the schools I had studied in the late 1960s, though those schools also were in the political spotlight as they traversed the early years of a voluntary desegregation plan. Thus participants had more reason to be cautious in talking to an outsider. Again, whether because of the political need to control any information about the school, or because of my more impressive credentials and more sage appearance, the principals in all three schools were far more generous with their time in talking to me than were the principals in the earlier study.

Race is clearly an important variable, especially when the setting under study is a multi-racial one. All my studies have been in desegregated schools. In all, racial differences and intraracial and interracial interactions have been one of the subjects in which I was

interested, though not the primary ones. I am white. In 1967-1968 other whites were much more open with me about their racial attitudes, especially their prejudices, than they were in 1979-1980.[2] Blacks were perhaps more forthright in discussing the prejudices to which they are subjected. But in both settings, both races were cautious in attempting to assess my position before saying much. And in both my race clearly affected the kind and amount of information about both attitudes and practices to which I could obtain easy access. It seems clear that in a study in which race is a major topic under study, the optimal pattern of research requires an interracial team--though such a team will in itself inhibit some people's reactions to all its members.

Aside from those roles which one carries into the field visibly about one's body, one also carries in an identity associated with the setting which has led one to the research. One is a graduate student, a professor, or an employee of a private or public research agency. This identity is highly relevant to the fieldworker's role as it suggests the purposes to which the study will be put for either institutional or personal reasons. Staffs of schools are highly aware of these identities.

School systems within easy reach of universities are likely to be overwhelmed with a traffic of graduate students looking for a laboratory for their mandatory research. Though ethnographers may be fairly rare among these, they may also be regarded as especially demanding with their need for long periods of observation and for open-ended means of data gathering. Even when I did my first research in 1967-1968

I was greeted both wearily and warily by the system officer in charge of extramural research and the principals of the schools I studied. Some teachers, too, expressed weariness with research which took their time, yet seemed to have little outcome either for that school in particular or for schoolchildren in general (cf. Paddock and Packard, 1981). The city where I currently am working has fewer graduate students to deal with, but with the passage of time, the school personnel have become sophisticated about the uses of research data to build careers. Some spoke to me cynically, and some pleasantly and supportively, assuming the use of the study to build my own future.[3] One teacher interrupted my usual explanation prior to a formal interview of the purposes of the research for basic understanding and a mixed audience of sociologists and practitioners to ask abruptly, "What do you get out of this?"

Somewhat startled, I replied, "Do you mean me, personally?"

"Yes," he replied without embroidery.

"Well, I have to publish, lest I perish," I replied with a light laugh.

"OK" he said and indicated we should start the interview. He was apparently satisfied both that I was honest and that I had an understandable personal stake in the enterprise which did not identify me with a local faction or agency of which he wished to be careful. He assumed that personal purposes would be paramount and looked for ones which would be harmlessly self-centered.

To the degree that he was identified with the school, he probably should not have been so easily satisfied. Literature on the sociology of the professions more widely as well as that on the reporting of ethnographies more narrowly (Seeley, 1964; Warren, 1980) has indicated

that pressures to report one's findings in ways that will please particular professional subcultures or will create a stir of news within the profession, if not in the society, are great. However, this is a pressure to which ethnographers are neither more nor less susceptible than researchers using quantitative methods.

Interpretive Perspectives Attributable to the Role of Fieldworker

Just as the latent roles any field researcher must bring along into the context to be studied may facilitate or impede the gathering of data and will probably shape it in either case, so the role that one develops qua field researcher will facilitate some kinds of data gathering and some kinds of insight while impeding others. Ethnographers familiar with the literature I have cited know that one of their first duties is the careful scrutiny of deed, word, and thought for signs that their cultural or personal biases are affecting either their interaction with others in ways which will skew the data or their interpretation of the data they receive in ways which will skew analysis. But sometimes, it is difficult to distinguish bias from insight. Research conclusions do not necessarily first appear in the form of logical reasoning. Much can be learned from attentiveness, with proper skepticism, to the fieldworker's feelings as well as thoughts. The positivistic bias of much social science research tends to blind us to the insight that can be gleaned from feeling.

I used observational methods in my first study not out of any philosophical argument for the primacy of process, but simply because the problem I was initially interested in--the effects of congruence and incongruence in definitions of legitimate classroom authority--demanded

observations of actions rather than verbal summaries of actions. I quickly discovered that teachers and students differed vociferously among themselves over these definitions. I carefully scrutinized my behavior and initial analyses for signs of personal bias on these issues. I came to the conclusion that I did have a personal predilection favoring the side in the debate which gave more rights of participation to children. That side also defined educational ends to include the development of curiosity, of social responsibility, and of individual initiative as well as subject matter mastery. I made a very conscious effort in reporting the study to be fair to both sides.

As I worked on my current study of magnet schools I was once more alert for this bias, though the issues of concern were somewhat different. Here I found that I differed from many of the staff and parents and some of the university colleagues with whom I would discuss the broad issues at stake. I gave primacy to the social atmosphere of each school and to the quality of interpersonal relations as an indicator of its effects on students. Others were more narrowly concerned with demonstrable academic progress, a variable which I found important, but not so central as they did.

It was not until after I had left the field and was well along in the analysis, that I realized that what I was interpreting as a bias to be resisted was in fact a trained insight to be fostered. Classrooms are very private places. Strong norms establish them as the territory of single teachers who let other adults in rarely and often reluctantly. Thus, ethnographers such as myself have a truly rare experience in sitting in the students' seats hour after hour, day after day, long

enough to affect their own mood and their experience of the overall shape of their lives. They alone do so with the sensibilities and the capacity to articulate their experience of adults rather than of children.

My "personal bias" for more participatory, broadly designed, education which developed during the course of the first study, and for signs of students' personal comfort in the situation and their easy mixing across lines of achievement and race which developed in the second study, were not pre-existing biases at all. Rather they were responses, generalizations, gradually drawn out of my participation in the school as an observer. They were felt, emotional, responses, yes, but I would now define them as reasonable conclusions from research. Ethnographers not only can see and measure differences in the quality of life between and within schools, (Clement, 1978; Rist, 1979; Rutter et. al., 1979), but they also can experience them and feel their effects on themselves (Cusick, 1973; Peshkin, 1978) and thus "know" their weight upon one's feelings of self-worth and one's willingness to enter the situation on a daily basis.

Thus, it was not just an interesting sidelight of the research that I blushed and wished momentarily to disappear in a sixth grade class when I had my pencil and notepad in action as the teacher prissily[4] told the class,"Everyone put down your pencils. When I speak I want your full attention." Similarly, my suppressed and suffused feelings of anger and moral disapproval when a teacher shouted at a student over some tiny infraction, or when a teacher greeted a class belligerently at the start of a class hour were probably very accurate parallels of the feelings the children experienced in the same situations.

My excitement and pleasure as a class sought together to solve a riddle posed by the teacher reflected my being caught up in the collective mood. In the same way, my genuine enjoyment as well as my sense of participation in watching a group of eleven to fourteen year olds put on a musical play told as much about my sense of belonging in a school which bent great efforts to produce a common sense belonging as it did about the (genuinely) high quality of costuming and performance mustered by the young thespians.

But, if my experience taught me well the force of the personal and social character of life in classrooms and schools, it taught me little about children's academic progress except through the media of formal indicators such as test scores or indirect indicators such as teachers' estimates of student progress.[5] Thus my methods may have led me to underemphasize the academic aspect of school experience. Significant gains conferred by any research methodology are generally offset by some kind of loss.

The point to be made here is the general one that the participant observer role involves participation. That participation teaches a lesson through the medium of the researcher's own experience including his or her responses to that experience. Too mistrustful a search for bias in its assumption that the researcher is a faulty instrument, because a unique one, overlooks the fact that the observer is a trustworthy instrument because he or she is also an example of the universal, an instrument very like the persons about whom one wants to learn. Where the researcher's experience is one rarely shared by articulate spokesmen, it is especially likely to yield insights for a larger

community. To acknowledge that those insights may, and probably will, be selective is not to deny their value.

The role of the participant observer breeds other special perspectives which work in more subtle and complex ways. The role is unusual, difficult for most people to understand, vaguely defined, and in some ways deceptive. During field work all of these characteristics create considerable strain for both ethnographers and the people they study. Some create strains which continue through analysis and writing.

First, fieldworkers must constantly introduce and explain themselves as they enter the field. Most of the people they are studying will not understand what they are doing. Repeated explanations will be necessary, and even with these workers are likely to find their role assimilated to some more familiar model. If the model is that of administration spy or government agent,[6] participants' understandings of the role are likely seriously to damage rapport. Thus, fieldworkers must find nonverbal as well as verbal ways to reassure respondents and demonstrate their trustworthiness, respect for confidentiality, and neutrality in internecine conflicts.

But most important for this discussion, the field researcher's role is in some ways a deceptive, or at least artificial, one no matter how fully and straightforwardly its occupants describe their purposes and interests. Though people know ethnographers are there to study them, when the ethnographers are at all skillful, participants gradually "forget" this fact as a researcher becomes an accepted and ordinary part of the social landscape. Even without great effort on the researchers' part, ties of simple human affinity will grow up between them and some of the participants. But under ordinary circumstances, researchers

will assiduously cultivate such ties, for it is often remarked in the literature (e.g. Colvard, 1967; Schofield, 1977) that the fullest and most fruitful pieces of information are given in the context of relationships of ease and mutuality.

Ethnographers as persons are subjected to multiple strains by these role demands. Not only must they be constantly "on" defining themselves, guarding against misinterpretation, judging ambiguous situations, and forming an emergent agenda. Also they must ingratiate themselves with others personally and appear to interact spontaneously so that participants are not inhibited by their presence, while they in fact reserve their own feelings and use occasions and relationships which others treat as ends in themselves for their own instrumental purposes. They are always in the situation or the relationship but not "of" it. And even when their spontaneous emotions do arise and they develop feelings of affection and loyalty, they must hold these feelings at bay as sources of bias not only in their perception of those persons with whom they have developed ties, but also of others to whom their "friends" may be emotionally or professionally opposed. Furthermore, researchers are always aware that an end will arrive when they withdraw from the field and even their most personal conversations and exchanges will be categorized as data (Daniels 1967).

Gans (1968) has insightfully argued that fieldworkers consequently suffer two sets of pangs. First, they live a socially marginal life, as they invest enormous amounts of social and emotional energy in a situation to which they can never allow themselves to belong. (See also Khleif, 1974.) Second, they are constantly guilty

as they use personal relationships to instrumental ends, no matter how high their purposes may seem. Gans argues that these two emotions can easily lead to a compensatory "overidentification" in which participant observers make up for their marginality and their guilt by seeing themselves as the champions of the studied group in the larger world, as persons who will interpret its true character and dispel negative stereotypes.

I would agree that this phenomenon is very real. I experienced it in the study of magnet schools particularly in one school. In that school the administrators expressed the most open concern that a study might damage their reputation before I entered, yet the whole staff made me the most personally welcome and gave me information most freely. After about six weeks in the school I found myself identifying so much with the staff as they faced a cutback in federal funds, that I reduced my presence at the school for a while to regain my outsider's perspective.

But the same set of strains in the researcher's role may create a contradictory dynamic in field researchers' feelings toward the groups they are studying. They may express their sense of marginality and guilt by disidentifying in a similarly exaggerated way. Their duty to participate in a situation they find in some way repugnant to their values, to stand by outwardly nonjudgmental while participants engage in behavior they find morally or aesthetically objectionable, may lead them to vociferous expression of their criticism upon leaving the field. Even when participants' practices give researchers no moral qualms, if participants make them feel unwelcome and resist their

efforts to collect data, researchers may develop a negative animus towards them.

Both forms of response may be experienced by the same researcher in different studies.[7] They may also be experienced by a researcher as he or she confronts different factions in a single setting or different settings in a study of multiple settings. When I moved from the school I just mentioned to another blessed with much better resources but with much lower faculty morale and lower levels of faculty effort and sensitivity to students, I was never able to muster as much empathy for any of the groups in the school as in either of the other two schools. And there were times when I found myself consciously fighting feelings of disidentification.

These psychological dynamics of the ethnographer's role are more complex then a simple congruence or incongruence between the ethnographer's values and those of the groups under study. They arise from the personal consequences of social interaction and they can run very deep. If ethnographers do not remember all the injunctions for self-analysis and skepticism toward the sources of their conclusions, the psychological dynamics of their role can shape their understanding and their description and analysis of a setting on more personal than empirical or intellectual grounds.

But it is important to remember that ethnographers' involvement with their research subjects do not make them different kinds of people from others. It is altogether possible that survey researchers also develop sympathetic and unsympathetic responses to certain kinds or classes of people. And if these responses to persons are divided along

the same lines as differences relevant to the research topic, such as social class, race, gender, or role occupancy in formal organizations, substantial bias in their interpretation of quantitative findings can result. While ethnographers' deeper involvement with the subjects of their studies may intensify their relationships with them, and thus their feelings, it also makes it easier for them to change those feelings as they come to empathize with initially alien groups (Gans, 1968; Van Maanen, 1981).

Latent Roles Specifically Related to Educational Policy

Ethnographers in educational settings have some distinctive issues to deal with as they consider the latent roles they bring both to field work and analysis. All of us have been students, and most researchers have taught, even if only at the college level. Most of us are parents of children who will be, are, or have been in elementary and secondary schools. Quantitative researchers bring the same experiences to the framing of research questions and the analysis of data.

It was in this area that some idiosyncratic and partially accidental experiences in my latest research brought me to some new insights. The magnet schools of Heartland, where I had recently moved, seemed tailor-made for my research interests. But I hesitated slightly before applying for a grant for the project because my oldest son was in the first grade in one of the magnet schools. With my positivistic training in sociology it seemed to me that such a close personal tie with the system might bias my perspectives or taint my observer's role. However, since he was in an elementary school with a different educational

specialty from any of the middle schools I planned to study, and since he would have two more years of elementary school at the close of the study, the two aspects of my involvement in the school system seemed safely insulated from each other.

Fate, and my naivete about the turmoil of arrangements in newly desegregating school systems, quickly caught up with me. I reluctantly yielded to pressure to run for the presidency of the ailing parent group at his school, thinking that it would involve no activity beyond the school walls.[8] At its August meeting the school board, without prior warning, closed the school and combined its students and staff with those of another magnet school which had not been filled. To make matters worse, they took a parallel action with one of the middle schools I had proposed to study.

I had a personally difficult choice to make. However, since I did not know whether I would be funded for the study and the demands of my research role were vague, while the responsibilities of my role as PTO president were relatively clear and the need of the mostly working class students for the services of the highly successful school compelling, I plunged into the political fray.[9] Parents of the two schools joined in an intensive three week effort, well covered by the local media, to change the board's mind. We failed. I enrolled my son in the neighborhood school where he had attended kindergarden and withdrew from school politics.

In the course of those three weeks of meeting with other parents, school board members, and representatives of the media, I was not a participant observer but a total participant (cf. Gans, 1968). As

Gans and others have found, total participation teaches one about the emotional quality of participants' experiences. I never could have understood these emotions as well with even the most intensive participation as an outsider. And I gained access to political knowledge shared among parents and with parents by school board members who had their own political reasons for supporting our cause. Such information would have been much less readily available to me as even the most patient of interviewers.

There were thus gains in knowledge from my participation that I did not anticipate. There were, of course, also lingering effects. Though I had feared a negative impact on my access to the schools, I found instead that my political participation seemed to smooth my entree with one principal of a middle school perennially threatened with a similar transfer. (I told her of my role, lest higher officials interfere in any way because of it.) The experience certainly paved the way for long and open conversations with the parents of the school which had been transferred along with my son's--even though they became divided into opposing factions during the following year. My background did make the principal of that school sporadically guarded in talking about issues where parents had an emotional stake, lest my relationship with them ally me with them or provide a pipeline of information. I listened to school board meetings on the radio and postponed my interviews with central office and board personnel until the end of the field work when my face and name --never very highly visible --had had time to fade from their memories. Though others told me that high officers in the central office put my research role and my parental

role together, they did not mention the fact to me.

The effects of this experience on my own perspectives were probably more important than its effects on my relationships with others. I became highly sensitive to the fragile political position of magnets and quick to see signs of the erosion of their special character. I also became very sensitive to the political and nonrational character of district decision making. The latter phenomenon, in particular, has been documented in other contexts (Cohen, March, and Olsen, 1972; Firestone and Herriott, 1981; March and Olsen, 1976). My experience thus helped me quickly to recognize organizational and political processes which were important in the magnet schools' relations with their environment, but it may also have led me to exaggerate their importance.

After both my sons, the younger then a kindergardener, had spent a year in the neighborhood school following the closing of the school I mentioned, it became clear to me from their experience there that structural factors gave the magnets a great advantage both in academic instruction and in interracial relations. For the following year we moved both boys into magnet elementary schools. I now had to admit to myself that I had a very real personal stake in the continued existence and full support of the magnet schools. I had to be very careful that that stake did not distort my understanding of the relations of the magnets with the rest of the school system.

Further experience with the research brought me to see that I had another role which affected my perceptions in the schools. That role was the one of city dweller. The issue of the city-suburban split began to become pressing during these years in many contexts which had

nothing to do with schools. Issues of taxation, of sewer extension and repair, of control of choice riverfront property, and of freeway construction and mass transit all pitted city interests against suburban interests.

One day at the first research school, teachers who lived in the suburbs were debating with those who lived in the city the wisdom of a recent ruling that new teachers must reside in the city. The suburban teachers held that concerned teacher-parents could not fairly be asked to subject their families and children to city conditions including city schools. City teachers held that the city is a healthy environment and the schools provide good education.

The implications of this debate for the teachers' belief in their ability to affect their students were obvious if one assents to the view that expectations of effectiveness affect both levels of effort and indirect messages sent to children. The conversation therefore was important to me as an observer. As a person concerned with effects of schools on children, it also seemed to me that the city teachers were more likely to be effective. But I found myself inwardly cheering for the teachers supporting city residence for more than these very good intellectual reasons. I was after all a city dweller with a large financial commitment in a house of which I had grown fond. And my children were in city schools and likely to remain there.

Similarly, when a local magazine ran an article decrying high rates of failure and dropout in the Heartland public schools, the highest, the article remarked with horror, in the whole state, my first response was to check the authors' address. Clearly, Heartland's

schools can only be evaluated by comparison to other cities of similar size and class and racial composition, not by comparison to its relatively prosperous rural state which contains no other city with Heartland's size and its social conditions. The article had made a clear methodological error, but I perceived it immediately, in part because of my heightened identity as a city dweller.

Nonetheless, I found that identity conflicting with some of my other roles. One day, with my professorial role foremost, I mentioned some tentative generalizations in a casual conversation with a sympathetic colleague, also a city dweller. I spoke confidently of "urban schools" having certain problems. "Are you sure those are especially urban problems?" he asked gently. It was immediately evident to me that they were not. But I had spoken with a certain easy condescension toward urban schools, as I identified with academia.

The importance of my own roles and my struggles with them lies in the fact that I have gradually come to see that the dramatic character of my parental involvement and the current evidence of city-suburban tension in Heartland have simply made me aware of a process which is universal. If I had lived in a suburb or a bordering university town and had my children enrolled in school there, I would have had no less of a stake or a bias in studying the city schools. For I would still have made semi-conscious comparisons between my children's schools and the ones I was studying. I still would have asked the unconscious question, "Would I want my child in this setting?" And though I might have disidentified rather than identified with parents who would put their children in such schools, I still would have had a parents'

feelings about the matter.

I am not saying only that researchers, like all persons are socially located and thus have a perspective rooted in a time, a place, a culture, and a set of roles (cf. Berger and Luckmann, 1966). That is true. But in the matter of urban schooling we also all make choices, whether consciously or subconsciously, which place us within very lively controversies. Those who live within commuting distance of cities, especially, make a choice to live in the city or outside it. And in making that choice--even if it is done over issues of acres of grass or capacity to influence local government--we decide whether or not to share the fate and the services the city offers.[10] Since the major service offered by local government is education, the decision of where to live is often importantly a decision about where children will go to school. Thus any one whose children have passed the age of five has a history of choice in public education with which he or she must live. Those of us who write about education, whether our data be qualitative or quantitative, will have perspectives shaped by that choice. We may wish to justify our actions or we may wish to compensate for them by supporting a different perspective. But those of us who are parents can not escape the fact that we have made real life commitments and that those commitments will shape our perspectives toward educational issues. We ought, at least in the privacy of communion with our typewriters, to admit that fact before we set words on pages and to be sure that those perspectives do not unduly shape the conclusions we reach in the public sphere.

This issue is complicated by a peculiar paradox in American education.

We agree as a society that the public schools should be an avenue for equality of opportunity. They should offer equal resources and an equal opportunity to every child. Public schooling should not be used for private advantage by those with more political or economic power. But one does not have to go anywhere nearly as far as the radical critics of education to see that in fact the schools do differ according to the economic and political power of parents. Inequality in per pupil funding between suburban and urban districts is only one, easily measureable, sign of this difference. As federal funds are withdrawn it will become ever more evident. Probably even more important is the unequal distribution of skills in the student mix as schools reflect segregation by social class in suburbs and even in individual school attendance areas.

We accept this situation because at the same time that we believe in equality of opportunity as a principle for the society in general, we consider it "natural" that parents will want to give their own individual children the best start possible--that is a better start than some one else's. But if we use public schools in select communities to this end, we are supporting unequal opportunity as we do so. And even if we use private schools tc this end, we are depriving public schools of one of their most important educational resources, well-motivated children with good skills, as part of the student mix.[11]

Researchers who deal with education, then, are in the difficult position of having made choices in an area of paradox were self-interest and one set of societal values routinely come into sharp conflict.[12] As a consequence they experience structural pressure toward less than total

harmony and clarity of thought on the subject. Individuals are nonetheless responsible for clear thought and carefully reasoned analysis, despite these social pressures toward confusion.

CONCLUSION

Ethnographers gather a much wider range of information than do survey interviewers. They are present among the people they study much longer and necessarily come to interact with them, so that both their outward social character and their inward social identification are bound to shape what they learn.[13] In response to this fact of the research process, ethnographers in both anthropology and sociology have developed a tradition of private introspection and public reporting through which they attempt to make explicit the ways in which their cultural commitments and individuality have shaped the research process.

It is part of the form of such research that ethnographers should investigate and report the ways in which their visible latent roles and their emergent roles as fieldworkers shaped others' responses to them. Similarly it is their responsibility from the first day in the field through the last day of writing to be aware of their cultural, subcultural, and individual assumptions. They must learn to recognize the ways in which these affect what they select to perceive from the varied reality about them. And they must be aware of how these can both reveal connections obscure to others and blind them to connections others could clearly see. This awareness should inform their efforts as they set about the task of weaving their data from the field into a coherent description and analysis of the processes at work there. It is then their responsibility to give a reasonable account of the research process

to readers who may then draw their own conclusions about sources of bias in the gathering of data and its analysis.

It behooves educational ethnographers to be aware of this tradition and to follow its canons. If they do, they may make as much of a fresh contribution to our understanding of schooling in the United States through this open discussion of the effects on all our findings of researchers' distinctive social locations and commitments as they do through the distinctive kinds of data their methods are able to uncover, analyze, and synthesize.

NOTES

1. In a small study in the interim (Metz 1978a, 1978c) my experiences were similarly intermediate.

2. In the intermediate study in 1975 in a small town on the border of the south, whites were only minimally self-conscious about expressing prejudice towards blacks.

3. It was interesting that even after I had told individuals who had asked about my purposes that I held the doctorate already, many spoke of my "paper" or my "dissertation". Research in this context is the most familiar for school people--and especially so when done by a woman.

4. That's how an adult would describe it after the fact.

5. This lack reflected my unit of analysis as well as my methods. Other ethnographic studies which focus on a few classrooms have dealt much better with academic progress (Carew and Lightfoot, 1979; Smith and Geoffrey, 1968).

6. Janes speaks of having been subjected to the "FBI complex" as the community he studied tried to make sense of some one who asked questions and studied documents all day (Janes, 1961).

7. Colin Turnbull, for example, is as direct in his condemnation of the Ik tribe (1972) as he is lyrical in his praise for the BaMbuti one (1961).

8. It is perhaps only fair to admit that with a preschooler still at home I had been professionally involved only part time because of my commitment to be my children's primary caregiver through their preschool years. I had also been active in the parent group of the school that

year, as the numbers were few and genuinely useful work needed to be done. I was--and to a lesser degree remain--more involved in my parental role than are many researchers.

9. I have described the school,the impact of the move, and some of the political and organizational context which led up to such precipitous action in Metz (forthcoming).

10. Lest I seem to be claiming moral superiority, I must add that my husband and I chose to live in the city primarily because it offered the most house for the money in a tight housing market. We did look in the suburbs and would not have turned down an attractive suburban house. It was only later that we fully recognized and came to identify with "our" side of the city-suburban controversy. Had we bought a suburban house our outlook on a number of issues might have turned out differently.

11. Murray Edelman (1977) has written about the presence and consequences of such contradictory political beliefs, used as convenient. He uses as his illustration our common "explanations" of and responses to poverty.

12. It is also important that each of us has received our own elementary and secondary education in more or less privileged settings. As researchers, we have experienced the advantages of attending selective and prestigious undergraduate and graduate departments or the disadvantages of attending less prestigious ones. These experiences also engender commitments which color our interpretations of educational institutions.

13. Survey interviewers also <u>interact</u> with the people they interview, even if briefly, and this interaction leaves its traces

in the answers recorded on the survey instruments. The literature on survey interviewing recognizes this fact and deals with ways to minimize its impact or to arrange its impact so that the interaction will not inhibit or skew respondents' statements. (Thus in a survey designed to deal with feelings about aging for each gender, one would be sensitive to the impact of the age and gender of the interviewer and would instruct the interviewers in the effects of certain kinds of language and action relevant to the issues at hand.) However, research reports of findings based on survey interviews less often disclose how these matters were handled or how they affected the research than does ethnographic research.

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